

PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	Interventions to treat Post Traumatic Stress Disorder (PTSD) in vulnerably housed populations and trauma-informed care: A scoping review
AUTHORS	Bennett, Alexandria; Crosse, Kien; Ku, Michael; Edgar, Nicole; Hodgson, Amanda; Hatcher, Simon

VERSION 1 – REVIEW

REVIEWER	Stubbs, Jacob L The University of British Columbia
REVIEW RETURNED	24-Jun-2021

GENERAL COMMENTS	<p>Overall, this is a well conducted scoping review. My main comments are to (1) add more synthesis of the results across studies in the results section (rather than describe the individual-study results) and (2) to describe some key takeaways or implications for frontline workers or care providers working in this population. A few small points are listed below:</p> <p>Background:</p> <ul style="list-style-type: none">- I would suggest not defining people as their living situation or disorder (i.e., “the homeless”), but rather something like “individuals who are homeless” o e.g., Page 3, line 15 (but also throughout) <p>Methods</p> <ul style="list-style-type: none">- Page 5, line 11: Does “... titles were screened in duplicate by two independent reviewers” just mean that titles and abstracts were screened by two reviewers? If so, just saying “titles were screened by two independent reviewers” is clearer to the reader, given that the word “duplicate” is also used to describe studies identified and screened multiple times.- Page 5, line 12: how many titles/abstracts were used in the pilot screening?- Page 5, line 25: a few extra sentences on how the authors organized the data (i.e., what were the key themes and issues?) would be beneficial- Page 5, line 40: the authors state that persons with lived experience were included in the design of this project. However, they also state that they were not involved with the scoping review process and that they will be involved in the “second phase of this project as informed by this scoping review”. Were persons with lived experience directly involved at all in this specific study? It reads as if they were involved with other research conducted by the authors but not with this review, and if that is the case, I would either clarify or remove the “Patient involvement” subheading.
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	<p>Results</p> <ul style="list-style-type: none"> - Table 1: an additional column describing the study population and/or recruitment avenue would be helpful to contextualize each study - Page 12, line 32: this subheading may read better as “non-trauma-focused psychotherapies” - Overall, the results read more as a summary of individual studies. The reader would benefit from more higher-level synthesis of findings, strengths, limitations across studies, rather than (or in addition to) a description of individual findings. <p>Discussion</p> <ul style="list-style-type: none"> - The authors highlight that a primary finding is that there is little evidence on how trauma-informed care for PTSD in this population should be delivered and whether it is effective. However, an expanded summary or discussion of what frontline workers, care providers, physicians, researchers, etc, can do for PTSD and cPTSD treatment and/or research given our current understanding reviewed in this study would be beneficial. Readers would agree that there should be more research on this topic to address the gap in knowledge highlighted by this review. But are the main implications for individuals working with this population? The authors also state that care providers and policymakers need more guidance working with this population – given the results of this scoping review (as well as practice guidelines, standard treatment approaches, etc), what would the authors guidance be?
REVIEWER	Roebuck, Ben Algonquin College, Victimology Research Centre
REVIEW RETURNED	22-Oct-2021
GENERAL COMMENTS	Well done. I enjoyed the article, which has a very well-described methodology. I have recommended minor revisions, but nothing especially time-consuming.

VERSION 1 – AUTHOR RESPONSE

Reviewer 1

I would suggest not defining people as their living situation or disorder (i.e., “the homeless”), but rather something like “individuals who are homeless”

Thank you for the feedback. This change has been made throughout.

Page 5, line 11: Does “... titles were screened in duplicate by two independent reviewers” just mean that titles and abstracts were screened by two reviewers? If so, just saying “titles were screened by two independent reviewers” is clearer to the reader, given that the word “duplicate” is also used to describe studies identified and screened multiple times.

The suggested change has been made in the text.

Page 5, line 12: how many titles/abstracts were used in the pilot screening?

Thank you, we have added the following line to clarify, “...all reviewers ran pilot screening on a random sample of 25 titles and abstracts and 10 full-text studies to identify and address any inconsistencies...”

Page 5, line 25: a few extra sentences on how the authors organized the data (i.e., what were the key themes and issues?) would be beneficial

We have noted that the data will be charted by intervention type such as trauma focused psychotherapies, non-trauma psychotherapies, housing interventions, and pharmacotherapy.

Page 5, line 40: the authors state that persons with lived experience were included in the design of this project. However, they also state that they were not involved with the scoping review process and that they will be involved in the “second phase of this project as informed by this scoping review”. Were persons with lived experience directly involved at all in this specific study? It reads as if they were involved with other research conducted by the authors but not with this review, and if that is the case, I would either clarify or remove the “Patient involvement” subheading.

Updated Thank you for your feedback. This subheading is required by BMJ Open, regardless of patient involvement. We have modified the heading text to clarify: “Persons with lived experience were included in the design stage of this project, but were not directly involved with the scoping review.”

Table 1: an additional column describing the study population and/or recruitment avenue would be helpful to contextualize each study. How about we relabel the column Gender and include something more about the population but also include gender?

The Gender Column has been updated in Table 1 to “Population” to include more details about the included participants.

Page 12, line 32: this subheading may read better as “non-trauma-focused psychotherapies”. The suggested change has been made in the text.

Overall, the results read more as a summary of individual studies. The reader would benefit from more higher-level synthesis of findings, strengths, limitations across studies, rather than (or in addition to) a description of individual findings.

Thank you for your comment. We have added the following to address this:

“Guidelines on the treatment of PTSD recommend trauma focused therapies as the most effective interventions. This review clearly identifies a research gap in the application of trauma focused therapies in those who are homeless. The strengths of the studies included in this review demonstrate that it is possible to deliver interventions in this population. However, an important limitation is the difficulty of keeping people in treatment with most studies reporting low engagement with treatment or low follow-up rates. Few studies described what proportion of people agreed to participate in the treatments but the small numbers in most studies suggest that engagement in treatment is difficult. Further the literature in this area is primarily focused on women and US Veterans.”

Discussion - The authors highlight that a primary finding is that there is little evidence on how trauma-informed care for PTSD in this population should be delivered and whether it is effective. However, an expanded summary or discussion of what frontline workers, care providers, physicians, researchers, etc, can do for PTSD and cPTSD treatment and/or research given our current understanding reviewed in this study would be beneficial.

Thank you for your comment. We have added the following to address this: “Research needs to clarify which components of trauma informed care are the most important and how to operationalize these. The main implications from this review for individuals working with this population are that there is no evidence that contradicts recommendations from existing guidelines that trauma focussed therapies are effective treatments for PTSD. Trauma focused treatments should be accessible to people who are homeless or vulnerably housed. The delivery of effective interventions should focus on trauma informed approaches with an emphasis on safety, choice, awareness of how trauma affects the acceptability of care and a strengths-based approach.”

Reviewer 2

No comments to address.

VERSION 2 – REVIEW

REVIEWER	Stubbs, Jacob L The University of British Columbia
REVIEW RETURNED	30-Nov-2021

GENERAL COMMENTS	No further comments.
REVIEWER	Roebuck, Ben Algonquin College, Victimology Research Centre
REVIEW RETURNED	06-Dec-2021

GENERAL COMMENTS	<p>Please note that somehow my original review was not forwarded to the authors even after contacting the journal. So here is the content of my original review. Having read your revised manuscript, many of the issues have already been corrected but there will be some minor issues to address. Please do revise what is relevant to the manuscript currently, and disregard content that has already been addressed. I am happy to recommend the paper for publication without additional review, pending minor changes.</p> <p>1. Original Submission</p> <p>1.1. Recommendation Minor Revision, recommended for publication without additional peer review</p> <p>2. Comments to Author: Ms. Ref. No.: BMJOpen - 2021-051079</p> <p>Title: Interventions to treat post-traumatic stress disorder (PTSD) in vulnerably housed populations and trauma-informed care: A scoping review [NOTE: Stress is missing from your current document title]</p> <p>Overview and general recommendation: This article provides a scoping review of interventions for PTSD with people who are homeless or vulnerably housed, with an emphasis on exploring interventions that integrate principles of trauma-informed care. The methodology for the scoping review is rigorous and well described, integrating multiple guidelines, the PRISMA-ScR checklist, and the protocol is registered with the Open Science Framework. The review was further completed using Covidence software, which helps to make decision-making more transparent.</p> <p>The review concludes that very few high-quality studies have been completed to assess the effectiveness of trauma-informed PTSD-focused interventions for people who are homeless or vulnerably housed. The authors recommend the development of mixed-methods randomized control trials that incorporate a high degree of flexibility for participants to engage in meaningful ways. Overall, the paper describes provides a valuable review of the existing research, strong methodology, and conclusions that point to further research. The paper will be of interest to homelessness and housing researchers, as well as clinicians in larger agencies who run programs to support people with their mental health. The paper also establishes a clear rationale for a larger clinical trial.</p> <p>At the same time, the paper includes subtle language that could be seen as stigmatizing to people experiencing homelessness or mental illness, though this is very easy to correct. The authors' critiques of the studies could also better consider the challenges of measuring PTSD in contexts where people experience ongoing trauma. I will further explain these comments below. As the paper is currently presented, I find the core of it to be well-written and an original contribution to the literature on trauma-informed care,</p>
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	<p>PTSD, and homelessness. I believe the paper should be published with minor revisions, without requiring further review.</p> <p>2.1. Major comments: (note that the line numbers no longer correspond to the revised manuscript).</p> <p>1. As noted above, the paper includes some subtle stigmatizing language throughout, specifically the phrases, “the homeless” or “the marginally housed.” This occurs throughout the document and could be easily corrected with a search and replace. From a sociological standpoint, this language implies an identity of homelessness or constructs a diverse group of people as an object. It would be better and more trauma-informed to use language that recognizes how homelessness is an experience situated within broader structural factors. This can be as simple as saying “people who are homeless or vulnerably housed.” (My personal preference is to use “people” rather than “individuals” whenever possible, but this is not required). Similarly, on p. 12, line 33 or 34, a study’s exclusion criteria are described as, “Participants were excluded if they were psychotic, suicidal, had been diagnosed...” There is growing consensus that using ‘psychotic’ in this way can be stigmatizing. See, for example, https://www.healthpartners.com/blog/mental-illnesses-terms-to-use-terms-to-avoid/ Even if that was the language used in the study, I recommend it be reframed as “excluded if they had symptoms of psychosis, thoughts of suicide, or had been diagnosed....” These comments are very easy to address, but I included them under major comments because of their importance.</p> <p>2. Reading through the findings from the studies, I wondered if any of the studies referenced acute stress reactions and how they might have differentiated between distressing trauma-related symptoms from incidents occurring more than 30 days prior to ongoing incidents of violence and insecurity on the streets. It looked like many of the studies that did not include a housing component did not reduce symptoms of PTSD, perhaps in part to ongoing trauma. Maslow’s hierarchy of needs (while recognizing limitations to the theory), would suggest that when basic needs such as food, safety, or shelter are not met, it is harder to work on self-actualization such as healing from trauma. At the same time, when people move into housing and reduce their substance use, they may be confronted with more intrusive traumatic memories that may have been numbed with substances previously. This could show higher PTSD scores for people who are starting to do the hard work of processing trauma. While the paper did provide an overview of outcome measures from the studies, I would like to see a little more consideration of measurement challenges in the limitations, and acknowledgment, to the credit of the studies reviewed, that measuring trauma outcomes with this population can be difficult. This could be addressed with an added paragraph in the limitations.</p> <p>3. In your conclusion, you mention that the way people who are homeless or vulnerably housed understand the concept of trauma-informed may look different from researchers. This is such an excellent and important point!!! I recommend that this be carried forward into the planning of your phase 2 project. Beautiful.</p> <p>2.2. Minor comments: (note that the line numbers no longer correspond to the revised manuscript)</p>
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	<p>2. Page 4, Lines 19-31. This paragraph has a few statements that need to be expanded or further explained: Line 19, "Health care poses a unique and difficult challenge...", Consider reframing to something like, "Accessing healthcare can be challenging for..." Line 21-22, "Being vulnerably housed also exposes people to further trauma and revictimization, such as..." Line 30, "The approach recommended in clinical guidelines is trauma-informed care" This needs a stronger link with the previous idea. One approach?</p> <p>3. P11, Line 5. The HOPE model is previously classified as a partial trauma-informed model, but not for the Johnson et al. (2016) study. Is this intended?</p> <p>4. P15, Lines 22-23. I'm curious about the finding that housing had a modest impact with low evidence. I suppose versus no impact from the non-trauma-focused therapies, that is an improvement. Again, there are reasons why measuring PTSD in housing programs might be difficult. Just a reflection.</p> <p>5. P15, Lines 39-40. "The impact of an intervention that is trauma-informed is most likely to be seen in increased rates of engagement and completion of treatment." Conjecture? A trauma-informed approach with too many sessions or spread over too long a timeframe may not be completed. I think that flexibility and building programs with people with lived experience can improve structures in a way that will improve completion, regardless of whether the program is trauma-informed.</p> <p>6. P15, Lines 51-52. "rated lower quality." I would be cautious of this interpretation since the quality of a study should be measured by its intended objectives and the suitability of the methodology. Calling a single case study low quality misses the intended value of the contribution. I think it is better to include the descriptions you have made, "the levels of evidence were rated for each study and most studies had small sample sizes or low rates of follow-up." This issue will also be addressed in the discussion of Figure 2.</p> <p>7. P16, Lines 4-5. Given that very few studies were trials of trauma-informed practices, I'd suggest reframing as 'a need for trials... The authors of the studies you have identified may be really valuable resources for building a clinical trial, which may be difficult if you characterize their work as 'poor quality.'</p> <p>8. P16, Line 20. "Managing PTSD and complex trauma can be challenging for many service providers..." It is most challenging for people who are navigating homelessness. It would be good to recentre trauma survivors here and make service providers secondary.</p> <p>9. P23, Figure 2. This is where the language "low-quality studies" seems most problematic, and could be reframed. Also, it is not clear in all cases what relationship the concepts on the outer circle have with the studies on the inner circle since choice or safety may be a component of any of the interventions. Finally, as with the rest of the paper, please change "the homeless"</p> <p>2.3. Minor typos: 1. P9, Lines 3-4: Women (50%), Is there some missing text here? 2. In the DSM-5, posttraumatic is one word, though I recognize multiple spellings.</p> <p>2.4. Concluding thoughts: This is a good paper with a strong methodology. All of the revisions I have requested or suggested are relatively minor in scope and can be easily addressed by the authors. I am happy to recommend the work for publication with minor revision.</p>
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VERSION 2 – AUTHOR RESPONSE

Reviewer 2

2.1. Major comments:

1. As noted above, the paper includes some subtle stigmatizing language throughout, specifically the phrases, “the homeless” or “the marginally housed.” This occurs throughout the document and could be easily corrected with a search and replace. From a sociological standpoint, this language implies an identity of homelessness or constructs a diverse group of people as an object. It would be better and more trauma-informed to use language that recognizes how homelessness is an experience situated within broader structural factors. This can be as simple as saying “people who are homeless or vulnerably housed.” (My personal preference is to use “people” rather than “individuals” whenever possible, but this is not required). Similarly, on p. 12, line 33 or 34, a study’s exclusion criteria are described as, “Participants were excluded if they

were psychotic, suicidal, had been diagnosed...” There is growing consensus that using ‘psychotic’ in this way can be stigmatizing. See, for example,

<https://www.healthpartners.com/blog/mental-illnesses-terms-to-use-terms-to-avoid/> Even if that was the language used in the study, I recommend it be reframed as “excluded if they had symptoms of psychosis, thoughts of suicide, or had been diagnosed...” These comments are

very easy to address, but I included them under major comments because of their importance.

The authors thank the reviewer for this comment. The authors have made the suggested language changes with the exception of the research questions, which uses “individuals” and have been left to match the published protocol.

2. Reading through the findings from the studies, I wondered if any of the studies referenced acute stress reactions and how they might have differentiated between distressing trauma-related symptoms from incidents occurring more than 30 days prior to ongoing incidents of violence and insecurity on the streets. It looked like many of the studies that did not include a housing component did not reduce symptoms of PTSD, perhaps in part to ongoing trauma. Maslow’s hierarchy of needs (while recognizing limitations to the theory), would suggest that when basic needs such as food, safety, or shelter are not met, it is harder to work on self-actualization such as healing from trauma. At the same time, when people move into housing and reduce their substance use, they may be confronted with more intrusive traumatic memories that may have been numbed with substances previously. This could show higher PTSD scores for people who are starting to do the hard work of processing trauma.

We agree that living in shelters or on the streets increases the chances of further trauma and associated “acute stress reactions”. None of the studies assessed or measured this. Experience of

recent trauma and reactions to it would be important in further research. We have mentioned this in the limitations along with the point below about outcome measurement.

While the paper did provide an overview of outcome measures from the studies, I would like to see a little more consideration of measurement challenges in the limitations, and acknowledgment, to the credit of the studies reviewed, that measuring trauma outcomes with this population can be difficult. This could be

addressed with an added paragraph in the limitations.

We have added a section in the limitations.

3. In your conclusion, you mention that the way people who are homeless or vulnerably housed understand the concept of trauma-informed may look different from researchers. This is such an excellent and important point!!! I recommend that this be carried forward into the planning of your phase 2 project. Beautiful.

Thank you very much for your comment. The lived-experience perspective will be a key part of designing the phase 2 project.

2.2. Minor comments:

1. Page 4, Lines 19-31. This paragraph has a few statements that need to be expanded or further explained:

- Line 19, "Health care poses a unique and difficult challenge...", Consider reframing to something like, "Accessing healthcare can be challenging for..."

The suggested change has been made.

- Line 21-22, "Being vulnerably housed also exposes people to further trauma and revictimization, such as..."

Expanded to include the example of increased exposure to physical or sexual assault as well as lack of access to basic needs.

- Line 30, "The approach recommended in clinical guidelines is trauma-informed care"

This needs a stronger link with the previous idea. One approach?

We have updated the language to say "One approach recommended..."

2. P11, Line 5. The HOPE model is previously classified as a partial trauma-informed model, but not for the Johnson et al. (2016) study. Is this intended?

All studies involving the HOPE model have been updated to be classified as a partial trauma-informed model.

3. P15, Lines 22-23. I'm curious about the finding that housing had a modest impact with low evidence. I suppose versus no impact from the non-trauma-focused therapies, that is an improvement. Again, there are reasons why measuring PTSD in housing programs might be difficult. Just a reflection.

The authors thank the reviewer for this reflection. We agree and believe it has also been addressed in our Conclusion where we note future studies should consider experiential evidence in addition to symptom measures.

4. P15, Lines 39-40. "The impact of an intervention that is trauma-informed is most likely to be seen in increased rates of engagement and completion of treatment." Conjecture? A trauma-informed approach with too many sessions or spread over too long a timeframe may not be completed. I think that flexibility and building programs with people with lived experience can improve structures in a way that will improve completion, regardless of whether the program is trauma-informed.

While we appreciate the reviewer's comment, flexibility is a key component of most trauma informed care practices. Designing flexible programs is part of trauma informed care so we stand by our comment.

5. P15, Lines 51-52. "rated lower quality." I would be cautious of this interpretation since the quality of a study should be measured by its intended objectives and the suitability of the methodology. Calling a single case study low quality misses the intended value of the contribution. I think it is better to include the descriptions you have made, "the levels of evidence were rated for each study and most studies had small sample sizes or low rates of follow-up."

This issue will also be addressed in the discussion of Figure 2.

The authors appreciate this insight into how levels of evidence were presented. The statements regarding level of evidence refer to the strength of any conclusions that can be drawn from a particular study either because of its design or how it was conducted.

6. P16, Lines 4-5. Given that very few studies were trials of trauma-informed practices, I'd suggest reframing as 'a need for trials... The authors of the studies you have identified may be really valuable resources for building a clinical trial, which may be difficult if you characterize their work as 'poor quality.'

There is a need not just for more trials but those that are well designed so that strong conclusions can be drawn from the work. We prefer to keep this sentence.

7. P16, Line 20. "Managing PTSD and complex trauma can be challenging for many service providers..." It is most challenging for people who are navigating homelessness. It would be good to recentre trauma survivors here and make service providers secondary.

We have added the following statement to try to centre the lived experience while also highlighting the clinical perspective this manuscript is meant to inform: "Managing PTSD or complex PTSD and navigating social support systems, including access to effective mental healthcare options, is a significant challenge to people who are vulnerably housed. It can also be challenging for many service providers working with a population where appropriate treatment approaches are poorly understood..."

8. P23, Figure 2. This is where the language "low-quality studies" seems most problematic, and could be reframed. Also, it is not clear in all cases what relationship the concepts on the outer circle have with the studies on the inner circle since choice or safety may be a component of any of the interventions. Finally, as with the rest of the paper, please change "the homeless"

The issues outlined in this comment have been updated in Figure 2.

2.3. Minor typos:

1. P9, Lines 3-4: Women (50%), Is there some missing text here?

There is no missing text – the sample was 50% women as reported.

2. In the DSM-5, posttraumatic is one word, though I recognize multiple spellings.

The authors have gone through to ensure consistent spellings. We have used Post Traumatic Stress Disorder when referring to PTSD and posttraumatic in all other instances.